

# Answer to comments from the Ad-hoc review committee

## Measurement of deeply virtual Compton scattering off Helium-4 with CLAS at Jefferson Lab

October 9, 2020

The committee met on July 2, 2020, to discuss our concerns about the manuscript. We are separating out our comments on English, wording, typographical errors, etc., as helpful suggestions for you, which do not need any response. However, there are a few areas of concern where we would like a response before we can approve the manuscript. In some cases, we would appreciate a direct answer or some clarifying information. In others, we recommend changes to the manuscript to make it more clear and easier to read. In any case, please note your response and any manuscript changes in a written document in addition to any discussion between the committee and the authors by e-mail, so that the response can be preserved in the official record.

1. As this is a long paper, giving more details about two published letters, we'd appreciate some statement as to whether or not anything in the analysis has changed between this paper and the two previous papers? Did the systematic error decrease? Did the central values of the results move in any case more than  $1\sigma$  of their former error. Perhaps it would be good to state the changes/improvements achieved in this new publication.

There was no change to the results. The statement at the end of the second paragraph clearly states that we are giving more details and do not mention any change or new analysis.

2. We have concerns about how you describe your approach to the  $\pi^0$  contamination, especially in the incoherent channel.
  - (a) In Figs. 14 and 15, bottom right panels, what is the definition of the  $\phi$  angle? Is this the  $\phi$  angle of the  $\pi^0$  ?  
Yes.
  - (b) What kind of input distributions went into the  $\pi^0$  generation to produce the  $\phi$ -dependence of Figs. 14 and 15.  
This is given in eq. 3.6 and tab. 3.1 of the analysis note, there is no  $\phi$ -dependence in the generation. We decided not to go to this level of details in this paper as the parameters have no relevant physics significance.
  - (c) We feel that it would be more useful to see the  $\phi$  distribution calculated (from simulation) from the single detected photons, i.e., what the  $\phi$ -distribution of your actual background looks like.

The intent of this figure is to compare the simulation to the real data, to assess the quality of the simulation. What you propose would not be more useful, it would serve

a different purpose: showing the shape of the background. We do not have a way to show this background from experimental data so it would not allow to assess the same information. We provided some of the numbers by email, but are not convinced it highlights anything important about the analysis. The article is already going into a lot of technical details, probably more than most comparable long paper. So we prefer not to go to that extend.

- (d) In lines 280-282, you write "To make the correction on the DVCS BSA, we assume that the exclusive  $\pi^0$  production has no such asymmetry. This has been checked with the exclusive  $\pi^0$  production data, for which no significant level of BSA was measured." There are couple of issues here. First, this claim is contradicted by CLAS data on protons, for example <https://arxiv.org/pdf/0711.4736.pdf>, which observed 5-10% BSA over a wide kinematic range. Second, even if you check with your own exclusive  $\pi^0$  data and see no BSA, this claim is only so good as the precision of your check, and there must be some systematic uncertainty associated with the possibility of a BSA below your ability to detect. This needs to be explained in the paper.

The results are not contradictory, our target is half proton half neutron and will experience, at least, some nuclear effects. There is clearly no solid ground to build a correction from the existing data. Moreover, our CLAS measurement shows results consistent with 0, so if we were to make a correction it would be very small. On your second point, there is indeed a systematic error associated with the  $\pi^0$  subtraction as specified in the Tab. 1. We clarified in the text that this correction includes a possible undetected asymmetry in the exclusive  $\pi^0$  data.

- (e) It would be helpful for you to write a few words explaining the differences in the assumed  $\pi^0$  distributions for the coherent and incoherent cases.

There are no assumptions on this, we adjusted the parameters to fit the measured distributions. We clarify this point in the text of the paper.

3. Regarding the final state interaction discussion starting at approximately line 100, the issue is a nucleonic final state interaction issue, and we are not sure why DIS is mentioned here as a reference. There must be people who spent their time investigating the  $A(e, e' 0 p)X$  nucleonic final state calculations, for example, that might inform us on these effects. Perhaps adding a more relevant reference would help.

We added a modern reference about quasi-elastic FSI. We welcome suggestions if you know of more relevant publications on the topic.

4. In Section 4.1, your choice to use the present tense here is quite jarring, especially since you don't make a distinction between the original CLAS, the upgraded CLAS-12 detector. Please consider switching to past tense here and making that distinction explicit. Furthermore, it would be good to state that these data came from the EG6 run period, and give the year that they were collected.

This section has been modified.

5. Line 147: "Its historic use to measure DVCS in many different configurations made it an ideal place for this new DVCS measurement." This is not an informative claim. CLAS can be ideal for DVCS for many reasons, but you're pretty much giving a tautology here.

The information is that CLAS has been used for other DVCS measurements in the past. It seems to be a relevant information when showing results for DVCS on a new target.

5. Section 5.1: "In particular, it serves for the identification of the protons." Are you referring to the time-of-flight detection of the electron or the proton? Perhaps separate sections for electron ID and proton ID are warranted. Furthermore, you have no section on the ID of  $^4\text{He}$  in the RTPC. We know that this is covered in the RTPC NIM, but a paragraph would be nice.

We made the change and clarified the detection of protons and helium nuclei.

6. Line 238: "In principle, a selection based on two or three variables can be used to guaranty the exclusivity of the process." In exclusive DVCS, you measure the momentum vectors of 3 particles of known ID, i.e. 9 quantities. Exclusivity requires both energy and momentum conservation, i.e. 4 constraints. Where do "2 or 3" come from? That is a vague statement.

This is incorrect, one constraint is enough, you can ensure exclusivity by only cutting on missing energy, if it is 0 the process is exclusive. Due to the effects of resolution, this is usually never a good solution and 2, 3 or more variables are used, often missing mass and missing momentum for instance. Anyway, we agree the statement was vague and not very useful, we changed this sentence completely.

7. Line 302: "As it is not obvious which solution is best. . ." What is obvious is that this is not an issue of correct or incorrect, but one of possible bias introduced to your BSA from your poor  $t$  resolution. Please rephrase the paragraph in these terms, and state what systematic uncertainty is introduced from the limitations in your ability to reconstruct  $t$ . If this effect introduces negligible uncertainty on the BSA, that's fine, but make that argument clear.

We added a sentence about the fact that no error is associated to this study.

8. Table 1 is mildly misleading, simply because the beam polarization is a relative uncertainty, while the others are absolute uncertainties (if we understand correctly). Perhaps it is better to pull beam polarization out of the middle of the table and put it somewhere else so it can be understood as distinct. That 3.5% is a multiplicative factor, right?

We changed the order of the lines and the presentation in the table to make it clearer.

9. Eq. 25 appears to have a mis-print.  $x_A$  should be  $x_A$ .

Done.

10. English and Typographical suggestions.

All done.